the vessel is moved," Mr. Jordan supposes that this will *always* be so; and that the tendency of ocean-water to be left behind is the great source of tides and currents. But if he will try the experiment of *continued* rotation, especially with a vessel havng not a smooth but an irregular interior, he will find that *after a time* the water rotates as fast as the vessel itself, and partakes of its momentum. Were it otherwise in the case of the Earth, no rock could withstand the abrading power of the mass of water which would be constantly impelled against its eastern face—not only on the surface, as in the case of the trade-wind current, but at its greatest depths. That Ocean-water not changing its place northwards or southwards, *does* fully partake of the Earth's motion, and *does not* tend to lag behind, is proved by the exceptional cases in which a flow of water moving towards either Pole tends *eastwards* in virtue of its *excess* of easterly momentum, and in which a flow moving towards the Equator tends *westwards* in virtue of its *deficiency* in easterly momentum. The *Challenger* temperature-sections of the Atlantic show this to be the case with the cold-stratum beneath the Gulf Stream, which comes to the surface along the Atlantic sea-board of the United States; a similar "cold wall" has been found by our Naval Surveyor, Capt. St. John, to intervene between the Kuro Siwo (which is the counterpart of the Gulf Stream in the Pacific) and the eastern coast of Japan; and the recent researches of Dr. Meyer have shown that even in the North Sea a like upward movement of the colder under-stratum is distinctly traceable along the eastern shores of Britain, and still more on the eastern slope of the Dogger Bank.

It would be quite useless to either follow Mr. Jordan through his detailed application of a theory which is so completely baseless, or to examine into the validity of his criticisms of the views of others. He is obviously a man possessed, like the notorious upholder of the earth's flatness, by a "dominant idea" which nothing will dispel; and all we can do is to warn our readers that his book is good for nothing, except as a warning example of misdirected ingenuity.

ANIMAL MECHANICS

Principles of Animal Mechanics. By the Rev. S. Haughton, F.R.S. (Longmans, Green and Co.)

THIS formidable volume has four languages on the very title-page, and bristles throughout with numerical calculations, analytical formulæ, and geometrical constructions. When, in addition, we record that it contains anatomical details, teleological postulates, hints on the best mode of hanging, &c., it will be obvious that no one man can be expected to be able to pronounce upon its value from more than a few of the possible points of view.

We are told in the Preface that the object of the work is to show "the mutual advantages obtainable by anatomists and geometers from a combination of the sciences which they cultivate. Anatomists will gain by the increased precision which numerical statements must give to their observations, and geometers will find in anatomy a new field of problems opened out to their investigation." Surely there is nothing new in this statement! Every anatomist worthy of the name strives after the greatest attainable precision in those observations in which it is requisite, and many able mathematicians have treated of anatomical problems. But passing this over, we are obliged to say that Dr. Haughton's mathematics are barely such as are calculated to attract the anatomist. When writing for a class of persons who, at the best, rarely know more than the merest elements of mathematics—surely it would be well to use the simplest processes which will suffice. This is not Dr. Haughton's method; he rather acts on the principle of making an investigation as showy as possible by the introduction of an immense quantity of quite superfluous analysis. This is, no doubt, calculated to impress the majority of readers with an idea of the author's profundity; and, though even very ordinary mathematicians will find no reason to share this impression, we cannot understand the necessity for putting such a threatening barrier in the way of the poor anatomist who wishes to understand the reasons here assigned why muscles have the particular forms which it is part of his business to examine, describe, and classify.

Excellent instances of this peculiarity of the work may be given in great numbers, but one must suffice. Take the investigation in p. 239, which is given to prove that no work is done by a quadrilateral muscle when one of the bones acted on revolves about a certain given point. The result given in the text follows instantly from the most elementary geometry, if a single additional line be inserted in the woodcut; always, however, providing that the reader is prepared to allow the following postulates, which may, perhaps, not be very readily assented to, but which are as necessary for the elementary geometry as for the pompous analysis. The first is, that when muscular fibres are extended, as much *negative* work is done by them as there is done of positive work when they contract by the same amount! The second is, that in a plane quadrilateral muscle the fibres run in lines which, if produced, would all meet in the intersection of the lines joining the ends of their places of attachment to the bones, and that they are *uniformly* distributed radially from this point. Postulates of this kind are, indeed, very common throughout the work.

The three great features of novelty in the work, so far as we can perceive—in addition to the very numerous, and obviously careful, determinations of the weights, &c., of corresponding muscles in various beasts, birds, and fishes—are the *Law of Fatigue*, a grand teleological *Postulate*, and the *Principle of Least Action.*

These are enunciated as follows, the third as applied to the heart:—

"When the same muscle (or group of muscles) is kept in constant action until fatigue sets in, the total work done, multiplied by the rate of work, is constant."

"The Framer of the Universe (*Δημιουργός τοῦ κοσμοῦ*) has constructed all muscles upon the principle that each shall perform the maximum of Work possible for it under the given external conditions."

"The arrangement of the fibres of the heart must be such as to allow each fibre to contract to the fullest extent required by the law of muscular contraction."

As a simple comment on the first of these, rendered very instructive by the insight it affords us into the general cogency of the author's reasoning, take the following:—

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> "If any man wishes for a simple proof of the inferiority of the endurance of his muscles as compared with those of a woman, let him carry a child on his arm for the same time that his wife or nurse can do (*sic*) with ease, and he will find himself much fatigued."

In this striking passage, for "a woman" read "another man;" for "wife or nurse" read "coalheaver;" for "child" read "sack of coals;" and for "arm" "back." It is still obviously true! Many other, perhaps even more remarkable, forms of this statement will present themselves to the intelligent reader.

We had marked for comment or quotation a great number of passages, but considering the characteristic qualities of the specimens we have given, we think the reader who wishes more may safely be left to seek them in the work itself. Perhaps the most curious point we have not yet alluded to is the author's calculation of the force which can be exerted by the abdominal muscles. In his first publication of this astonishing result he adopted *seriously* a quotation from Sterne which Duncan, writing on the forces employed in parturition, had used (in its original intention) as a mere joke; and in the work before us, in spite of all that has been done, especially by Duncan and Schatz, since that first publication of his, Dr. Haughton still gravely writes:—

> "Thus, we see that, on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner."

It is only necessary to explain that this is assigned not as the whole pressure on the *surface* of the child, but merely as the component in the direction of its motion!

POLAR EXPLORATION

The Gateway to the Polynia: A Voyage to Spitzbergen. From the Journal of John C. Wells, R.N. With numerous Illustrations. (London, 1873. 8vo, pp. 355.)

EVERY fresh book on the Arctic Regions helps to awaken the dormant interest of the public in the question of Polar Exploration, and from that point of view this volume commands our attention. From no other, however, can we recommend it. The "rapid sketch of Arctic voyages" contained in the introductory chapter is rapid indeed—we might also add vapid—and it is followed by a disquisition on things in general in which some of the statements are true and a few of them new, though the new and the true do not seem to be always successfully combined. Of course it was not to be expected that the masterly summary of the progress of northern discovery given by Richardson in his well-known "Polar Regions" should be excelled or equalled, but we had a right, we conceive, to look for a few more details than we get of the American, German, and especially of the Swedish expeditions executed since Sir John's work appeared. But even letting that pass, we should have been contented with a plain narrative of Capt. Wells's own "Voyage," whereas we have nothing of the sort. We are told, it is true, that he sailed in the yacht *Sampson*, that he left this country in May 1872, and, after reaching lat. $80\frac{1}{2}^\circ$, returned in the following September—facts which any of our readers may find if they take the trouble of looking back into our columns; but of the incidents and results of the voyage we are afforded only the most vague outline, drawn in a confused and book-making way. One remarkable and suggestive fact is to be noticed. The name of the owner of the yacht never appears in regard to this voyage! Little bits of what may once have been written in a journal pieced together with stories more or less (and rather less than more) connected with the subject, such as that of the building of Scalloway Castle (imperfectly told by the way)—yarns spun by old whalers and sailors—scraps of zoology, botany, geography, and meteorology (some of them incorrect)—long extracts from Parry's well-known "Voyage"—the whole jumbled into one chaotic mass, from which it is difficult to derive any clear knowledge of what belongs to the writer of the "Journal," and what has been drawn from other authors. We are treated to certain woodcuts, the like of which were the wonder of our childhood, such as that of the Right Whale (p. 64); but whether Captain Wells saw a Right Whale, or knows one when he sees it, we don't profess to say, and this particular portrait is simply named "Whale." The cut representing "Whales' Food" (p. 82) is altogether wrong; whales would fare badly if they only swallowed such nourishment as the *Hydrozoa* there figured, and the author might have learnt better from old Friderich Martens, two hundred years ago. Shetlanders are said (p. 71) to be a "branch of the Celtic family." The Reindeer figured (to face p. 223) were certainly not drawn from Spitzbergen examples, and most parts of the book indeed might almost just as well have been written by a man who had never been to that country.

But perhaps all this may be looked upon as trifling. Capt. Wells's great object is to urge the claims of the Spitzbergen over the Smith's Sound route for future Arctic discovery. On this question much has been written and spoken; and though the opinion of experts is overwhelmingly strong in favour of the latter, the former is not to be dismissed in the off-hand way in which it frequently is. Impossible as it may seem, we wish to reconcile the adherents of either creed. Capt. Wells, we think, is not the ablest of advocates. He omits putting the point as strongly as it ought to be put, indeed his theory is utterly opposed to it. In his map all the space encircled by the 85th parallel is marked "Polynia," and an arrow-head obligingly informs us that "the gateway to the Pole" lies in long. 10° E. To force this gateway by steamer would seem to be his advice. Now we cannot agree with him here, for the idea of a great extent of perpetually open water, which is the essence of the notion of a *Polynia*, is a mere assumption, against which much seems to militate. Now there are two entirely different things for the Arctic discoverer to do. If his object be merely to reach the Pole by the cheapest and easiest means, our belief is that there is no way better than the Spitzbergen route, but one cannot expect to do it by water. The expedition should winter in the north of Spitzbergen, or on one of the outlying islets, and sledge-parties should be sent in early spring over the ice to reach the goal, and return with all possible speed. But if the object be to make a really satisfactory exploration, then the almost perfect agreement of Arctic authorities declares for Smith's Sound. It is possible that the Spitzbergen route might be accomplished by private enterprise, but for the other a Government expedition is essential. On parting with Captain Wells, we are

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